same time the apple was yellow with the spermogonia of a Roestelia, at the time immature, but which afterward developed into a form of R. penicillata. It might be asked why, judging from Prof. Halsted's culture and the New London specimens, we should not consider the R. penicillata to be the æcidium of Gymn. macropus, for Mr. Thaxter's culture, while it seems to point to a different conclusion, is not sufficient in itself. If we look at the opinions of European botanists we find that they differ very much with regard to R. penicillata. On anatomical grounds alone, some regard it as merely a form of R. lacerata. Others, like Winter, think it distinct. From their cultures, also, they have not reached a definite conclusion; for, while Oersted thinks that R. penicillata is the æcidium of G. clavariæforme, Rathay maintains that it is a form of Gym. conicum. Oersted considered that he obtained both R. lacerata and R. penicillata from sowing the spores of Gymn. clavariæforme on Cratægus and apples respectively, but it is claimed that he never really obtained the æcidia on apples but inferred that the spermogonia on apples must belong to R. penicillata. But such an inference is not strictly logical. In American cultures Gymn. clavariæforme was followed only by R. lacerata not by R. penicillata which is in confirmation of the views of those who are opposed to Oersted's conclusion. In other words, the undoubted Gymn. clavariæforme on J. communis in this country acts when sown just as that species is said by the opponents of Oersted's view to act in Europe. If we accept Oersted's view we must accept the view that Gymn. macropus of this country is only a form of Gym. clavariæforme which grows on J. Virginiana. This is the view of Schroeter, but it is difficult for botanists in this country, who have seen both species growing, to regard them as forms of a single species. It may be true, however, and the important point for our botanists to settle is, can the spores of R. penicillata be made to grow on both J. communis and J. Virginiana and produce on the former what we now call Gymn. clavariæforme, and on the latter what we call Gymn. macropus.

The Theory of Immunity from Contagious Diseases.*

D. E. SALMON.

The immunity which an individual acquires from the effects of a contagion, by passing through one attack of the disease which it causes, has never been completely and satisfactorily ex-

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plained. Various conjectures have been offered, but no one of these to my knowledge has been based upon sufficient direct and positive evidence to warrant its acceptance as a well established theory of immunity. Since the demonstration of the germ theory of contagion, it has been evident that there were, in a general way, three possible explanations of acquired immunity, viz: a substance might be formed in the body during the course of the disease which is unfavorable to the microbes; or a substance essential to the growth of these microbes might be excreted or in some way lost or destroyed during this period; or, finally, the living matter of the body might acquire the power to resist or prevent the growth of the microbes.

It is well known that Pasteur has adopted the second or exhaustion theory, and sustains it by his observations on the growth of microbes in culture liquids contained in flasks. If we sow chicken bouillon, he says, with the microbe of fowl cholera and after three or four days filter the liquid in order to remove all traces of the microbe, and afterwards sow this parasite again in the filtered liquid, it will be found powerless to resume the most feeble development. He assumes that there are but two hypotheses by which this fact can be explained: either the microbe has exhausted something from the culture liquid essential to its multiplication or it has added some substance which is unfavorable to it. To decide between these two possibilities a culture of the microbe was evaporated *in vacuo* without heat, and then brought back to its original volume by the addition of fresh culture liquid. He reasoned that if the growth of the microbe had been arrested in the culture by the formation of a substance which acted as a poison upon it, then the activity of the microbe would not be renewed after the addition of the fresh liquid since the volume had not been increased and all of the chemical principles were retained. As a matter of fact the multiplication of the microbe was renewed, and consequently the antidote theory was rejected and the exhaustion theory adopted.

Doubtless M. Pasteur's conclusion is correct as applied to the growth of microbes in flasks, but when we take into consideration the conditions under which such organisms multiply in the animal body, we find the elements of the problem very materially changed. The body is very different from a culture flask to which nothing gains entrance and from which nothing is eliminated. The insusceptible fowl is continually taking into its system fresh food which contains principles suited to the growth of the microbe in question. If the body is to be compared to a culture flask we should expect the immunity to be at the most of but a few days' duration, since the fresh nutriment should increase the capacity for growth in the one as well as in the other. Immunity from contagious diseases, when once acquired, however, does not terminate so soon, and generally persists for years.

The exhaustion theory is susceptible of being tested by direct experiment. If a fowl is insusceptible to cholera because it lacks some element essential to the growth of the microbe, then bouillon made by infusing the muscles of this fowl in distilled water should also lack this same element and would therefore be equally incapable of nourishing the germ. In February, 1881, the writer was investigating the subject of fowl cholera, and made this experiment; and he found that the proliferation of the microbe was just as vigorous in bouillon made from insusceptible fowls as in that made from susceptible ones (Rep. U. S. Dep. Ag., 1881 and 1882, p. 292).

Both the antidote and the exhaustion theory, consequently, fail when tested by direct experiment; indeed when we consider that there must be a different chemical substance exhausted from the body for each contagious disease against which immunity is acquired in the one case, or a different product for each disease added in the other case, the theories become at once improbable.

If we direct our attention now to the third or vital resistance theory, such discrepancies in regard to well established facts will not be found. Immunity is probably never absolute, but simply relative. Chauveau found that the Algerian sheep, supposed to be insusceptible to charbon, would succumb to that disease if a sufficiently large dose of virus was administered, and the writer found that fowls insusceptible to ordinary doses of cholera virus would contract the disease if the dose was sufficiently increased (*loc. cit.* p. 289). By turning these experiments in the opposite direction, I found that the effect of virus upon susceptible fowls varied to a certain extent with the dose, and a point was finally reached at which no symptoms of disease were produced, although some of the most virulent germs were introduced into the body (Rep. U. S. Dep. Ag., 1883, p. 48)

These facts indicate that the tissues of the most susceptible individuals are not suited to the growth of microbes when the functions of the cells are normally performed; because, if favorable, one germ introduced into the interior of the body would multiply just as it does in a culture flask and finally produce the disease with the same certainty as would a million. This not being the case, it is evident that by increasing the dose the resistance of the tissues is in some way overcome, the microbes multiply and the disease is produced. If the germs failed to multiply when a small number were introduced, because there was something lacking in the constituents of the body which is essential to their growth, it is difficult to understand how this unfavorable condition can be overcome by increasing the dose of virus; or if the failure to multiply was due to the existence of some substance which acts as a poison to the microbe, it is equally difficult to conceive how a large dose of virus would insure proliferation when a small one fails.

That the influence which prevents the multiplication of the microbes is connected with the vital activity becomes more probable from the fact that the bacteria of putrefaction, organisms closely related to the pathogenic microbes, are unable to reproduce themselves when introduced into the tissues; but they find favorable conditions for growth there as soon as the life of the tissue is destroyed.

With these various facts in mind, we are prepared to understand how immunity results from one attack of a contagious disease. The cells of the body are at first depressed in their activity or narcotized by the poison of the microbes, but after being subjected to its influence for a certain length of time they acquire a tolerance for it just as people begin a tolerance for tobacco and are able to smoke and chew it without inconvenience, although the first attempt made them deathly sick. Of course as this tolerance is gained the tissues resume their vital functions as before, the liquids of the body become unfavorable to the existence of the microbe and it perishes. From that time forward for a considerable, though indefinite and variable, period the animal enjoys an immunity from that particular microbe when introduced in limited doses; but just as almost any one can be made sick by sufficiently increasing the dose of tobacco, so the immunity of most individuals may be overcome by administering a very large dose of virus.

[The discussion of observations and theories made by Metschnikopf, Chauveau, Zülger, Riemschneider, Hiller, Pasteur, and the author, which bear upon the elucidation of the subject, but do not affect its general statement, have been omitted for lack of space.—EDS.]

If these conclusions are correct, then we should be able to develop immunity by introducing into the body the poisonous products of bacterial growth which have been freed from all living organisms. This result would be a most decided advance in the preventive treatment of contagious diseases. Investigations of this question have not been as numerous or thorough as is desirable. Pasteur found that his fowls which had been treated with the narcotic above referred to were still susceptible. The writer made many experiments with the same poison, which were also negative in their results. Law has published experiments with swine plague from which he claims positive results, but the number of animals operated upon is too limited to be at all conclusive, even if the details of the experiments were satisfactory, which is not the case. Quite recently in our experiments pigeons have been granted a very complete immunity from the effects of swine plague virus by treating them with cultures of the microbe in which all living organisms had been previously destroyed by heat. Up to this time, however, our experiments with pigs have only given negative results.

Although there are still some points in connection with this subject which greatly need experimental elucidation, it is believed that the theory developed in this paper is in accordance with the facts so far demonstrated. The problems of immunity have long been considered impenetrable mysteries, and if this theory does not prove in all respects correct it is hoped that it may at least be of some service to other investigators.

BRIEFER ARTICLES.

Dr. Gray's letter to the Botanical Club.—*To the Botanical Club of the A. A. A. S., at Buffalo:* I am unable to attend the ensuing meeting of the Association. But wishing to manifest my interest in the Botanical Club, and to show that I am not altogether an idler in the camp, I send herewith two small papers,* containing the result of some recent systematic work upon two familiar genera.

They are not readable papers, and therefore should not be doled out at your sessions, which will naturally be occupied with more interesting and discussable communications. I can not even make an abstract of them, which would be much more readable than the papers themselves. But the few lines prefatory to the essay on Dodecatheon will sufficiently explain that undertaking, and the result, which has given me no small satisfaction. For we have always felt confident that there were distinct western species, although I have never till now found a clew to lead toward the extrication of the various forms.

I am glad to find that only one really new name will be needed in the nomenclature of the five species which we have presented.

As to Violets, I make out thirty-three wild North American species, of which only eight are represented in the Old World. In two instances, namely, in the *Palmata* and *Blanda* groups, I have kept up recognized species, which almost every botanist believes to be confluent, but is yet not disposed to suppress.

* The paper on Dodecatheon published in this issue, and that on Violets in the next